

## Henry Norris Russell and the Expanding Universe

David DeVorkin

*Senior Curator, National Air and Space Museum, Smithsonian Institution,  
 Washington D.C., USA*

**Abstract.** Henry Norris Russell, one of the most influential American astronomers of the first half of the 20th Century, had a special place in his heart for the Lowell Observatory. Although privately critical of the founder for his pronouncements about life on Mars and the superiority of the Mars Hill observing site, he always supported the Observatory in public and professional circles. He staunchly supported Tombaugh's detection of a planet as leading from Lowell's prediction, and always promoted V. M. Slipher's spectroscopic investigations of planetary and stellar phenomena. But how did he react to Slipher's puzzling detection of the extreme radial velocities of spiral nebulae starting in 1912, and how did he regard the extension and interpretation of those observations by Hubble and others in following decades? Here we describe the arc of Russell's reactions, dating from Slipher's first detection, as an indicator of how mainstream stellar astronomers reacted to the concept of an expanding universe.

### 1. Introduction

Henry Norris Russell was one of the most influential American astronomers of the first half of the 20th Century (DeVorkin 2000). During his most active years, dating roughly from the eve of the first World War and lasting till the eve of the second, his influence was deeply felt not only as a frequent referee and arbiter of standards for American professional journals relating to astronomy, but as a spokesman for the profession, reaching popular and technically informed audiences in a monthly astronomy column for *Scientific American*. His column provided news of upcoming celestial events surrounding a simple star map, but it also became a bully pulpit for promoting the deeds and discoveries of astronomers worldwide, as did his two-volume textbook that was an introduction to the profession for over a generation of astronomers (Russell et al. 1938).

Russell's career spanned what Peter van de Kamp, Richard Berendzen and others have called, first, the "Galactocentric Revolution" and then the "Acentric Revolution" (van de Kamp 1965; Berendzen 1975). History will remember the 20th Century as the period when these revolutions in the human comprehension of the universe, in conjunction with the establishment of relativistic cosmology by Einstein and others, were discovered, elaborated and reinforced.<sup>1</sup>

Today, these concepts are in place, but how did those who were born and raised in a static stellar universe react at the time? Observational astronomers rose to the chal-

---

<sup>1</sup>An excellent summation of present historical knowledge, with an extensive bibliography, can be found in Smith (2009).

lenge, pragmatically conducting observational tests suggested by Einstein or inferred from his theories.<sup>2</sup> Those without the means to make the delicate observations, or without the inclination, however, had fewer options to participate: among them, either ignore the implications and continue on studying the stellar universe, or incorporate the implications when they might be useful.

Russell was in the last category. The Astrophysics Data System suggests that Russell invoked the term “expanding universe” only twice in refereed publications from 1931 - 1940, less frequently than de Sitter, C. A. Chant, W. H. McCrea and G. C. McVittie (9 times each), Eddington (4 times) and Hubble (3 times).<sup>3</sup> And distinct from the others (save Chant, who was acting in his capacity as editor of the *Journal of The Royal Astronomical Society of Canada*), Russell did not try to contribute to the new dynamics other than to speculate on its implications for the formation of planetary systems.

For these reasons, recounting how Russell reacted first to V. M. Slipher’s observations of the extreme velocities of the spiral nebulae and then to their elaboration and correlation with distance by Hubble and others, will offer insight into how “normal” astronomers engaged the new framework. Of course, following Russell alone will not provide a full historical picture, but considering his influence, his example is a good place to start, on the occasion of this celebration of the centennial of Slipher’s initial detection of what ultimately was interpreted as the expansion of the Universe.

## 2. First Reactions

In April 1920, the Dearborn Observatory astronomer Philip Fox wrote his friend V. M. Slipher, congratulating him on receiving the 1919 Lalande Prize of the Paris Academy for his nebular work. Fox also took the occasion to recall for Slipher how astronomers reacted when, at the 16th meeting of the American Astronomical Society (AAS) in Atlanta in December 1913, he read one of Slipher’s first announcements of the great radial velocities of a few bright spiral nebulae. “It has been a matter of very great gratification to me to see the recognition which your splendid work has been receiving,” Fox (1920) began, but then pointedly recalled:

I believe you remember that at the time of the Atlanta meeting of the Astronomical Society I presented your paper on the high velocity of the nebulae, and it was greeted with some expression of incredulity, especially on the part of Professor Russell, to which I made a heated rejoinder... I note that he makes a confident reference to your work, however, in “Some Problems of Sidereal Astronomy,” published by the National Research Council.

In the some four years between Russell’s initial reaction to Slipher’s work and when he prepared his influential essay “Some Problems of Sidereal Astronomy,” he came to accept the velocities as measured quantities. As his essay was intended to be a referendum on the state of astronomy, pointing to work that had to be done, he clearly

---

<sup>2</sup>There is an extensive literature, from, for instance, Wright (1966) to Crelinsten (2006).

<sup>3</sup>There were 224 hits total using the Astrophysics Data System’s full-text search (Beta version), searching on refereed papers with synonyms disabled.

identified the velocities of the spirals as a problem that needed to be addressed in the context of the “Sidereal Problem.” He, along with J. C. Kapteyn, A. Eddington and most mainstream astronomers, regarded the defining problem of sidereal astronomy as the spatial and kinematic arrangement of stars in the visible universe. The Sidereal Problem was then the search for order, trying to understand Kapteyn’s two star streams as the motions of particles in a spiraling vortex (Paul 1993).

What of the spirals then? Were they part of the stellar system, or external to it? Russell was aware of the problem, but to appreciate where they (or any unexpected observation) fit in his larger scheme of things, we need to appreciate the state of the profession of astronomy at the time. This was a time of war, and for some, including Russell, there were questions of what was critical for the future of the profession after the war.

The most contentious issue for the National Research Council (NRC)<sup>4</sup>, was what would be the nature of science after the Great War. In George Ellery Hale’s view, would international relations be re-established? Who was in, and who was out?<sup>5</sup> Hale organized the deliberations around disciplines, and asked Russell to take the lead for astronomers, concentrating on what the nature of the discipline should be like. As Russell (1917b) explained his position to Harvard’s E. C. Pickering in November 1917,

[T]here are two sides of astronomical research, one of which has to do with the collections of facts, and the other with their interpretation.

The former was routine; the latter was not, and this was why, Russell (1917a) claimed, none of Pickering’s Harvard staff had been asked to write reviews for the NRC. Pickering’s own earlier reports, he pointed out to his old patron, had already covered the routine aspects. Now the other side needed airing:

[I]t is upon studies of this sort that the future advances of any science must very largely depend.

Considering that it had been Pickering who made the vast holdings of Harvard spectra and magnitudes available to Russell in past years, which led to his landmark study of the relations between the characteristics of the stars, Russell’s views could be thought of as insensitive.

Russell indeed gave highest marks to interpretation, but he was always very deferential to those who collected the most interesting and useful observations, like Pickering, and indeed like Slipher, for it was upon their work that his career was based. His patronage of Slipher and Lowell Observatory needs to be understood in this way (as does his patronage of other observatories like Mount Wilson). Russell’s essay (Russell 1920c) had to wait till war’s end, and there he expressed his opinion very forcibly:

The main object of astronomy, as of all science, is not the collection of facts, but the development, on the basis of collected facts, of satisfactory theories regarding the nature, mutual relations, and probable history and evolution of the objects of study.

---

<sup>4</sup>Of the National Academy of Sciences

<sup>5</sup>See Kevles (1995, 1971, 1968).



Figure 1. Starting in the late 1920s, Roger Lowell Putnam, the Observatory Trustee (r), was receptive to Russell's (l) advice and intellectual patronage. Photograph by Clyde Fisher, circa late 1920s, Neg. #280362, courtesy Department Library Services, American Museum of Natural History.

When those facts were collected properly, and presented clinically, they had to be dealt with. Indeed, for Russell, observation guided theory as much as theory informed plans for additional observations. This was a very new idea for American astronomers, one that few could assimilate. In fact, "Some Problems" was an appeal to observational astronomers to appreciate the power of actually designing observational programs that answer specific questions. Very much due to Percival Lowell's problem-oriented style, the Lowell Observatory staff were more receptive than most.

Russell's feeling was that one of the most important results of a wider sampling of spiral radial velocities was to better determine solar motion: best determined once radial velocities of many nebulae are secured in all directions. This was a classical application he shared with most others who commented at the time.<sup>6</sup> So through 1916 to early 1917, at least, he expressed little interest in the nature of that larger realm, though he was rather open to the possibility that the nebulae were at great distances, even beyond the Milky Way. In fact, as early as 1903, in a *Scientific American* column, prior to the rise of the Chamberlin-Moulton encounter theory and still feeling safe in a planetary system rendered common by the Nebular Hypothesis, Russell (1903) went so far as to speculate that, viewed from a distance of a million light years, "our own Galaxy would appear as a spiral or a ring nebula, something like the Great Nebula...But it must be clearly borne in mind that the evidence available at present is too scanty to justify us in making any definite statement..." Thus for Russell in 1903, at least, the scale of the universe was not an issue.

---

<sup>6</sup>See Smith (1982).

He maintained this view, more or less. In a series of lectures in the Princeton Chapel in 1916 and 1917, Russell pondered the “Scientific Approach to Christianity” mainly to argue that there need be no antagonism between “Genesis and Geology” and that those at play were due to mutual misunderstandings that he hoped his lectures would put to rest. In one lecture, “What Nature itself looks like to the man of science,” Russell speculated that some of the fainter spirals could be upwards of 200,000 light years distant and that there was “No evidence that we have met with the limit.” Echoing the emerging findings of his former graduate student, Harlow Shapley, who was now at Mount Wilson, Russell (1916b) wholly accepted that “We are some 60,000 ly from the center of the galactic system is now known. . . .” He saw no inconsistency between the growing evidence of the nebulae being beyond the Milky Way and of the vastness of the Milky Way itself.<sup>7</sup> Nor did he express any concern for divergent time scales in Genesis and Geology at this time, evidently of a generation for which the “gap theory” was no longer necessary.<sup>8</sup> But what of the motions of the spirals?

### 3. Adjusting to Shifting Evidence

Indeed, one can easily trace Russell’s initial growing acceptance of the extragalactic status of the spirals through his *Scientific American* articles. Despite his initial skepticism, Russell was sufficiently impressed by June 1914 to encourage his *Scientific American* editors to invite Slipher to state his case for nebular motion and especially rotation, discussing its importance for “stellar and nebular evolution.” And by April 1915, Russell (1915) felt that Slipher had collected enough evidence for the phenomenon to devote most of his column to his work, opening his discussion saying “Among the most difficult of the yet unsolved problems of astronomy is that of the distances, dimensions and nature of the nebulae.”

Photography, Russell emphasized, had recently shown that the spectra of the spiral forms of these objects resembled stellar spectra, wholly unlike the spectra of the brighter “green” nebulae.<sup>9</sup> This meant they were either single stars cloaked in gas and dust, or assemblages of stars at vast distances. Russell then introduced Slipher’s accumulating observations of spiral radial velocities, now confirmed by “others” that showed them to be in the range of many hundreds of miles per second.

What was important for Russell was that the average of the radial velocities for the spirals was some 25 times the average for stars. If there was such great motion in the line of sight, one would expect there to be some observable translational motion as well. But Russell (1915) then noted that none of these nebulae showed any sensible proper motions, none exceeding 0.1 arc second per year, “though one out of every 4 stars exceeds this limit...”

Taking the planetary nebulae observed by the Lick astronomers, and the spirals observed by Slipher, as two classes, their measured radial velocities indicated statistically

---

<sup>7</sup>Shapley was coming to this distance estimate in late 1917. See Shapley (1918, p. 48).

<sup>8</sup>On the gap theory, see Moore (1986).

<sup>9</sup>At the time, astronomers still called diffuse nebulae, like Orion, green nebulae because of the visual dominance of a greenish tinge caused by the oxygen emission spectrum. White nebulae, like spirals, displayed continuous spectra. An explanation is given in: Thompson (2011). See also de Sitter (1932, p. 91).

even higher space velocities, which Russell translated into AUs/year. The planetaries travelled 15 AUs/year and the “white spirals” were travelling an astounding 120 AUs per year or more. Given this estimate, and the vanishingly small proper motions of these diffuse objects, or their stellar cores, they had to be at least 8 to 10 thousand light years away, but more likely in the ten to twenty thousands of light years. “The dimensions which the nebulae must have, to appear as big as they do at such distances, are astounding.”

Comparing Ejnar Hertzsprung’s recent estimate of the distance to the Small Magellanic Cloud, some 30,000 light years, Russell (1915) further speculated that the distances to those unresolved nebulae must be even greater:

...it may well be that we are on the brink of an expansion of our conception of the extent of the universe almost comparable to that which resulted from the first measurements of the distances of the nearer stars.”<sup>10</sup>

In September Russell (1916a) hailed V. M. Slipher’s observations of the great velocities of the spiral nebulae, which, along with the work of the other “Pacific Coast Observatories...has so greatly widened, at a single bound, the limits of distance at which our investigations may operate.”

But by late 1917, influenced somewhat by Shapley, who was excitedly reporting Adriaan van Maanen’s provocative conclusions about the rotation of spirals, Russell (1917a) started reversing his views. Responding to Shapley in November, Russell accepted van Maanen’s observations, which immediately threw doubt on the Island Universe theory: “But if they are not star clouds,” Russell mused, “what the Dickens are they?”<sup>11</sup>

Russell never overtly criticized van Maanen’s results. Indeed, since 1916 he had gained high respect for his astrometric abilities, convinced that van Maanen’s deft re-analysis and refinement of stellar parallaxes, especially his analyses of internal errors, was a very strong step forward in that delicate art. He looked upon van Maanen as a new breed of astrometrist, more adept at mathematical analysis than his predecessors, like Benjamin Boss or even J. C. Kapteyn.

As much as Russell evidently respected van Maanen’s abilities, his early results for rotation in spirals were so extreme that Russell remained cautious, despite Shapley’s promotional efforts. However, in October 1920, after van Maanen reported directly to Russell that he had found much the same rotational properties for another spiral, M 33, Russell expressed both excitement and great relief:

I have been waiting with great interest to see what you would get on additional spiral nebulae and this confirmation of your early discovery is most gratifying.

Russell (1920a) looked forward to more confirmations from Mount Wilson; in conjunction with Pease’s spectroscopic efforts, he encouraged van Maanen to obtain distances to these objects, so that they all could proclaim:

---

<sup>10</sup>Hertzsprung (1913, p. 204) converts a parallax of 0.0001 seconds of arc into a distance of 3000 light years, where it would be closer to 32,600 light years. Russell (1915) related it as 30,000 light years.

<sup>11</sup>Also quoted by Smith (1982, pp. 36-7).

Good-bye to Island Universidus!

Privately to van Maanen, he was even willing to play down the nagging question of the intrinsic brightnesses of various novae events seen in the spirals by Curtis at Lick, admitting that they were dim compared to galactic novae. They might well be intrinsically dim, Russell speculated, which he felt was well within bounds for discounting this odd class of object.

Thus in the two years between the time Russell first drafted the essay for the NRC, and when it was finally published in October 1919, and reprinted in *Popular Astronomy* in 1920, van Maanen's rotational proper motions had already raised serious questions about the existence of a hierarchy beyond the Milky Way, questions Russell was evidently happy to raise. Russell clearly wanted van Maanen's results to be confirmed, and so in his essay he went so far as to suggest, without mentioning de Sitter, that the distances the light had to travel from the spirals might also be causing the effects Slipher was seeing.

Shapley, of course, was wholly convinced by his own work on the distances to globulars and their association with the Milky Way, but it was not the deciding factor for Russell. Even without the confirmation that would come in October 1920 from van Maanen, Russell exhibited his enthusiasm in his National Academy essay, concluding for the moment that the distances to the spirals, and their intrinsic sizes, could not be "extragalactic" largely owing to van Maanen's remarkable proper motion measurements.

Russell's rejection of the extragalactic framework came after, as Robert Smith and other historians have related so well, Slipher's continuing efforts, now bolstered by Pease's spectra with the 60 inch, had won many astronomers over. There was strong evidence that spirals were not solar systems in formation: they were not stars. To Hertzprung and others these speeds meant that the spirals could not be bound to the Milky Way. International acclaim soon followed: In his Presidential address at the Annual General Meeting of the Société Astronomique de France in September 1917, even in the depths of war, the Count de La Baume Pluvinel (1917) rejoiced that much work had been done recently, especially noting Slipher's spiral radial velocities which "give a further proof of the independence of the spiral nebulae of our Universe."

In the early 1920s, therefore, Russell remained an agnostic over the meaning of Slipher's velocities. He never doubted the observations themselves any more than he doubted van Maanen's measurements; at every turn, Russell indicated that Slipher's careful work had to be factored into any discussion, for just as van Maanen had proven to be a pioneer in refining astrometric technique, Slipher had certainly done so for spectroscopy. In 1920 Russell became especially excited by Slipher's recent work on the Orion stars (Slipher 1919), writing to Adams that it "looks to me as if the luminescence of the nebula was due to some influence emanating from the hot stars within it..." This was another piece of evidence that "the nebular lines are produced by known atoms, under unknown conditions of excitation" (Russell 1920b). Russell never doubted Slipher's observations, and supported them through speculating about what they revealed about the stars, something Slipher rarely if ever tried to do.

Several months before Hubble's revelatory announcement of the distance to M31 based upon Cepheids of some 285,000 parsecs, Russell gave a series of popular lectures in February 1924 at the University of Toronto. During the 14th and final lecture, on the "other classes of great nebulae" - those of spiral and globular forms, he very much followed the evidence van Maanen offered for 7 spirals showing they were in

rapid rotation, throwing off material, very much in line, Russell believed, with James Jeans' theoretical models. Given all the evidence now at hand, Russell concluded, the spirals themselves could not be more than some 10,000 light years distant, which was not large compared to the size of Shapley's galaxy: "... if [van Maanen's] measurements are right, these things cannot be anything at all like as big as the Milky Way." The Andromeda nebula, however, was for Russell (1924c, p. 259) still one of the most "remarkable objects known to astronomical science." Given its enormous size and luminosity, and its apparent violent motions throwing off matter, it was a clue to formative processes in his universe.

Russell's highly convoluted presentation appears in hindsight to be due to his attempt to reconcile the wild observations at hand. But in fact he was most influenced by how, in his mind, Jeans' evolutionary model provides "a beautiful picture of the spiral nebulae" as evidence once again of his cherished nebular hypothesis. "It looks as if we have stages of evolution shown" Russell believed; Jeans could even account for the lumpiness of spiral arms. But Russell admitted that a very recent paper by Jeans which "came out only a week or so ago shows that when we want to be quantitative it is much more difficult." Jeans' latest model suggested, in fact, that spirals become more tightly wound with time.<sup>12</sup> Russell (1924c, p. 265) characteristically reminded his audience that science was tasked with how spirals "got into their present shape" but was not equipped to say: "...how they got there, because of that we do not know anything at all. It is no business of science to explain how matter got into space; only supposing there was matter in space, what would be the history of it." As Russell (1924c) concluded, "Only a portion of Genesis is accessible to the realm of science."

Everything changed, of course, by the end of the year and Hubble's work announcing the distance to M31. Russell heard the news first from James Jeans when they happened to meet in New Haven sometime in the fall, and then later in November. Finally, Hubble reported directly in December and Russell (1924a) responded heartily: "They are certainly quite convincing...It is a beautiful piece of work..."; Russell urged Hubble to send his paper to the Washington meetings of the American Association for the Advancement of Science in February 1925, if for no other reason than his paper would no doubt "bang that \$1000 prize." Russell read Hubble's paper at that meeting<sup>13</sup> saying nothing of the implications this new result had on van Maanen's measurements.

Jeans and Russell did meet in New Haven in August 1924, presumably because there were two major meetings starting with the American Astronomical Society, held at Dartmouth and timed specifically to coincide with the special annual meeting of the British Association for the Advancement of Science in Toronto. Russell reported on both in his *Scientific American* column, highlighting the recent studies of Yale's E. W. Brown on the osculating orbits of "particles composing the arms" in spiral nebulae. Both Jeans and Russell later acknowledged that they spoke of Hubble's observations and their implications for Jeans' theory of spirals in New Haven. And after Jeans returned to England in late October he wrote to Hubble based upon his conversations with Russell, sending a copy to Russell, setting out points that Jeans believed supported Hubble's new estimate of the distance to M31. Jeans made his points rather

---

<sup>12</sup>Jeans (1923) discusses the tightening of the arms.

<sup>13</sup>Smith (1982, pp. 111-120) and Berendzen et al. (1976, Chapter 5) have explored in some detail the impact of Hubble's first paper on Cepheids on the acceptance of van Maanen's rotational measurement work.



dramatically. Using Hubble's new distance to M31, and F. Pease's spectroscopic observations of its rotational motion, "we found a period of rotation of 17,000,000 years." Then Jeans (1924) speculated to Hubble: "Suppose that Van Maanen had measured motions in M.31 and had announced this period. If I had calculated the parallax by the method I used for M.101 on p. 217 of my "Cosmogony" (assuming, as I did there, that the condensations are 3'' of arc apart) the result I should have obtained would be  $(17,000,000/85,000) \times 5/3 \times 1,000$  parsecs = 333,000 parsecs. Which is near enough (rather too near!) to your estimate."

At first glance, it seems as if Jeans employed Hubble's distance post hoc but in fact, he found it by a very independent method: using Eddington's just announced mass/luminosity law for stars. Using the observed brightnesses of the average condensations in the nebula, their angular distances apart, and Pease's radial velocities, he found the distance to M31 to be 337,000 parsecs, employing Hubble's estimate of the absolute magnitude for the Cepheids of  $M = -4$ . To which Jeans (1924) again noted: "which again is near (too near) to your estimate"<sup>14</sup> and bluntly concluded: "In view of these calculations, I feel no further suspicions of the period-luminosity law, or of my physical interpretation of the condensations – on the other hand, I fear van Maanen's measures have to go."

Jeans was now able to show that Eddington's mass-luminosity law, as well as the period-luminosity relation, combined with Hubble's distances, supported his own concept of the nature of the spirals.<sup>15</sup> Here Jeans (1925) increased his earlier estimate of the distances to spirals like M31 (originally 5000 light years), in light of Hubble's and Shapley's work, to some 950,000 light years, and concluded: "If the same correlation [Eddington's M/L relation] is assumed to hold for the stellar condensations in a nebula, we can dispense with v. Maanen's measurements altogether and (in theory at least) determine the distance of the nebulae from the observed stellar magnitudes of its condensations." Russell (1924b) agreed, writing Jeans hastily in early November 1924 that "the agreement of the new data with your theory is beautiful..." Russell did not elaborate, however, saying there was nothing he could add off the top of his head.

Throughout 1925, Jeans explored the ramifications of Hubble's distances, clearly delighted with the new framework. Russell attended to his solar and stellar spectroscopic studies, experimenting with the emerging explanatory framework from quantum physics to find useful links between the laboratory and the stars. He could not have relished Jeans' enthusiasm for Eddington's mass-luminosity relation, of course, because it meant that, in Russell's own words (Russell 1926), it put his theory of stellar evolution "in a state of chaos." Later Russell (1928b) admitted openly that "[t]he path of this idea is strewn with the wreckage of abandoned theories," and that the field was at an impasse. In like manner, even though Russell could not have been comfortable with the implications of Hubble's vast distances, when he reported on them for *Scientific American* in March 1925, he also credited Shapley's distances to globulars and hence his estimate of the size of the Milky Way as having "expanded our existing ideas of the universe ten-fold." He did this, evidently, to assert that Shapley's universe was still viable (see Figure 2). He now fully accepted the vast distance to M31, admitting that it "leads to conclusions which are enough to make even a case-hardened astronomer gasp." M31

---

<sup>14</sup>Also noted in (Smith 1982, p. 120).

<sup>15</sup>Jeans described his thinking in greater detail in Jeans (1925).



Figure 2. Russell and Shapley remained life long colleagues and friends. Here they are captured aboard a steamer tour at the 1938 International Astronomical Union General Assembly in Stockholm. Photography by Dorothy Davis Locanthi, Courtesy E. Segre Visual Archives, American Institute of Physics.

was now, at Hubble's distance of a million light years, at least 35,000 light years in diameter, which would have made it larger than the Milky Way on Kapteyn's old model. But, Russell (1925) hastened to point out, it was still only 1/10th the size of the Milky Way given Shapley's work. This lingering attachment to the idea of the Milky Way as being somehow special provides insight into the nature of Russell's personal universe.

#### **4. Russell's Universe**

In addition to his scientific writings, as noted above, Russell actively wrote and lectured on science and religion for a widening audience, starting on campus after he attained the status of a Princeton professorship in 1908, promoted to full professor in 1911 and Director of the Halsted Observatory in June 1912. He espoused liberal theological views, reflecting his upbringing as the eldest son of a New School Presbyterian pastor, and by the 1920s took on the role of a modernist, intent on arguing that there was much theology could learn from science in, what he was coming to accept evermore, was an age of relativism. Russell's efforts to explore the interface between science and religion thus promises further insight into his reaction to the concept of an expanding universe.

In 1925 he was invited to Yale to deliver the annual set of Terry Lectures, which he titled "Fate and Freedom." Keenly aware that Presbyterian theological orthodoxy was still struggling to kill the liberalism of New School reformers, Russell asserted his modernism, proclaiming that "The old hope of finding final and perfect statements of

the truth about a simple universe has fled.” In its place was a striving for closer and closer approximations to the truth, “a steadily increasing accuracy of approximation in the description and interpretation of an incredibly and magnificently complex universe.” In its constant striving, science had freed itself from absolutes, whereas theology had not (Russell 1927; Longfield 1991).

During these years, as he gained prominence, he maintained a lengthy correspondence with inquirers seeking illumination about God from the world of science. In late November 1928, he responded to the inquiries of a reader of “Fate and Freedom,” the Beacon Hill doyenne Margaret Deland, an American novelist and poet and particularly articulate inquirer. Deland, inspired by her reading, hoped her views as a “philosophically uneducated” admirer, in the spirit of Moliere’s “average mind,” might be of some use to the Princeton savant. The revelation she wanted to share, without attribution, was “that in this universe of evident order and purpose, God is still ‘becoming’.” Only in this way, she felt, could she understand that “our imperfections may be steps in His (I wish I didn’t have to use the personal pronoun!) process of ‘becoming’” (Deland 1928). Although she makes no mention of Alfred North Whitehead’s influence here, especially his widely read 1925 “Science and the Modern World” there are similarities with what he later espoused as “process philosophy.” If there is a connection and insights to be had, I leave them for others. Thanks though are due to Matt Stanley for raising this question during the conference.

Russell (1928a) responded at length, clarifying his position, and his universe:

I suppose it is because I am a physicist that I find it hard to think of God as “becoming.” Of course, we can none of us hope for a full understanding of the Power behind the universe. But those of us who study the universe itself, at least from the material side, do not find evidence of progress at the heart of things. There is plenty of development and evolution of individual parts of the universe, from stars to souls; but these are all particular cases of the operation of invariable laws of nature.

Russell took the laws of physics as invariant in a universe that, on the whole, was unchanging, though the things in it do change. Russell (1928a) thought of his “God as manifesting Himself rather than evolving.”

Just over a month later, in early January 1929, Russell’s unchanging universe was again challenged. Soon after he reached Pasadena in what had become a yearly trek cross country to advise and counsel, he heard the local news about what Hubble was now up to. In a long letter to Lowell astronomer Carl Lampland, who was then in residence at Princeton in a scheme Russell created that hopefully would better acquaint Lampland with mainstream astrophysics, Russell (1929b) reported that Hubble:

... has definite evidence, at last, that the radial velocities of non-galactic nebulae increase with distance ... the explanation “may” involve a “de Sitter Universe” or some new mathematical invention - it is too early to say...

Russell’s expression “at last” highlights the fact, as Robert Smith has well shown (Smith 1982), that the velocity/distance relation was very much in the air in the 1920s. But he mentioned it only in passing, devoting most of his long letter reporting on stellar astronomy and new instrumentation at Mount Wilson.

Within a few months, as Russell gathered his thoughts for his *Scientific American* column, the idea of an expanding universe did not sit too well, although, as usual, the observed facts were not in question. It was the implication. In a column titled “The Highest Known Velocity” Russell made much of Milton Humason’s tedious work, taking exposures over several nights, of between 33 and 45 hours duration, and the resulting “very pronounced distance-effect.” Regarding it all as very “strange” Russell asked “what does all this mean?” Could this be a “peculiar form of the theory of relativity suggested by .... de Sitter” or was it something wholly different. Conversations with Richard Tolman helped Russell appreciate how a de Sitter universe could look like a “now familiar” Einstein universe on small scales, but the two differed, as he reported to his readers, “in a larger view, [where] space and time have unexpected properties.” On small scale, two particles at rest would remain at rest save for a mutual Newtonian attraction. But at vast distances, such as those between the nebulae, they would recede from one another and scatter “ever more and more widely and recede from one another faster and faster.” Even though de Sitter’s model literally predicted what Hubble had found, Russell (1929a) concluded that it was “premature” to adopt de Sitter’s ideas: “The notion that all nebulae were originally close together is philosophically rather unsatisfactory...”

Russell’s reaction was distinct only insofar as he did not question the data. Others certainly did. At about the same time that Russell wrote his column, V. M. Slipher, along with his friends at Mount Wilson, notably John C. Duncan and Walter Sydney Adams, questioned the reality of Hubble’s and Humason’s observations. The fact that two nebulae in the Coma group had velocities differing by some 3000 km/sec made Duncan (1929) skeptical; he happily reported to Slipher that Adams felt this discrepancy was throwing “doubt on the de Sitter theory and making the whole situation more interesting.”

Hubble’s continued confirmation and strengthening of the velocity-distance relation had to be faced by astronomers, one way or another. I’ll end by discussing briefly how Russell dealt with it through the rest of his career.

The first edition of Russell, Dugan and Stewart’s 2-volume Astronomy textbook was completed in 1926 and published in 1926 and 1927. A second edition appeared in 1938 (Russell et al. 1938). In the first edition he outlined Slipher’s observations in some detail, and, not questioning the observations themselves, still asked what it all meant: “Whether this represents a real scattering of the nebulae away from this region where the sun happens to be is very doubtful,” Russell asserted in a smaller pitched paragraph (a tactic he used to highlight speculative sections), adding that some form of “the generalized theory of relativity” that space is bounded and “returns into itself” might explain the spectral shifts.<sup>16</sup> In the second edition a decade later, Russell voiced this same concern in his chapter on “Nebulae,” but in a later chapter on the “Evolution of the Stars” that had been extensively rewritten, he included a long passage on the problem of reconciling various time scales: the earth, the stars, and the universe. Here Russell did mention Hubble’s 1929 work in a supplementary section, but it was highly terse, clinical and with little elaboration. He made no mention of Lemaître.

Russell also wrote popular essays on Einstein’s theories of relativity and toyed with the visible consequences of intense gravitational fields for his *Scientific American* audience. Even though Russell never tried to contribute to general relativity or theories

---

<sup>16</sup>Russell (1927, p. 850)

of the expanding universe, he was fully aware of the potential impact both might have on modern astrophysics, writing about them with clarity and style, making them accessible to the general reader and rank-and-file astronomer alike (Kaiser 1998, p. 321).<sup>17</sup>

Russell's most revealing response to the expanding universe was how he used it to restore the possibility that we are not alone in the universe. Even though he initially was repulsed by the idea of the nebulae being closer together in the distant past, a rather surprisingly rash statement to make in a *Scientific American* column, once he had the benefit of years and time for rationalization, he realized that in fact he could see something useful in the framework.

Deprived of the Nebular Hypothesis by the popularity of the Chamberlin–Moulton encounter theory, Russell always looked for ways to reinstate the theory (see Brush (1978b,a)). On its influence on the ubiquity of planetary systems, see Dick (2001). The expanding universe gave him an opportunity. The solar system was formed, he argued in his 1935 University of Virginia lectures “The Solar System and its Origin,” at a time when the universe would have been much smaller than it is now, at a time when collisions were more probable. He thought more (Russell 1935), in fact, of the idea that the solar system was formed at a time when, some two billion years ago:

...a very great event occurred. This would have been the time *par excellence* for encounters of all sorts. Or it may be that a cosmic New Deal occurred, and that, just afterwards, matter was distributed more widely, but more thinly, through space, to settle down into the stars.<sup>18</sup>

This was the approximate epoch when the presently observed expansion of the universe had its start. It also agreed, he then thought, with the radioactive age dating of rocks, the tidal evolution of the earth-moon system, and was well within the possible lifetimes of the stars. In this distant epoch, the probability of encounters may have been far greater than today. Not only were stars like the sun larger than at present in his lingering view of the course of stellar evolution, but the universe was far more compact. Russell speculated that at this distant epoch, encounters and collisions could have been a common occurrence. Following a suggestion he attributed to Willem de Sitter, he was willing to speculate (Russell 1935, p. 25) that the stars themselves pre-existed this moment of crisis in the universe, many coming through it unscathed. Collisions would have been more frequent in the past, relieving all of the encounter theories of the burden of making mankind feel alone in the universe. Envisioning a time when “all the matter of the Universe was tightly packed together, perhaps in the one great Atom, which forms the starting point of Lemaître’s hypothesis,—which deserves no less respect because of its picturesqueness,” Russell (1935, p. 138) thus ended his lectures on a hopeful note for the ubiquity of life in the universe. In commentary in *Scientific American*, popular speeches, and radio talks, Russell harbored a deep desire to believe that human beings were not an accident in the universe. For a talk at Bowdoin College in Maine in April 1941, Russell relied on the humor of Gilbert Chesterton, who once defended his belief that he had been born, even though he had no recollection of it and could not test it.

Russell always remained optimistic that somehow, in time, with effort, discrepancies would be reduced, and consensus would come, and though consensus would come

---

<sup>17</sup>As Kaiser and others discuss, in the 1930s, Princeton harbored more interest in General Relativity than most American campuses, yet it was not a central topic in physics until the 1960s.

<sup>18</sup>pp. 137-8

a deeper appreciation of the universe we live in. He delighted in describing the process of science as building on what was, for him, “a tissue of approximations,” as he described it in his 1937 AAS Presidential address.<sup>19</sup> There was great heuristic value in the process, as long as physical insight was not lost, and the clues that observational data provided were faithfully followed up and tested for veracity, but never swept under the rug. Thus, throughout this period when others looked askance at the disparity in the time scale for the age of the universe computed from Hubble’s recessional velocities, compared to the ages of all the things in it as derived by radioactive determinations of the age of the earth, the ages of stars allowed by nuclear energy sources recently uncovered by Bethe and Marshak, the time scales from dynamical studies of binary stars and star clusters, Russell searched as usual for a silver lining. In his May 1940 James Arthur Foundation lecture at New York University, Russell could at least hope that the inference of Hubble’s evidence, best described, he thought, “a little speculatively” by the Abbe Lemaître, could make it possible to have a universe filled with planetary systems:

The principal attraction of this scheme is that it pictures a short but tumultuous time in the early days of our present universe during which all sorts of things which can never happen now might have occurred, such as the origin of the planetary system and ... the formations of the heavy radioactive atoms.<sup>20</sup>

Russell acknowledged that if this were so, then the ages of everything had to be limited to less than 2 billion years time. Even so, in concluding his lecture, Russell rejoiced that science had at least succeeded in showing that the time scale of the universe can be measured in billions of years, not millions, “and in very few of them” as concluded from “four independent lines of evidence.”<sup>21</sup>

But this was as far as he was willing to go. He ended his essay acknowledging that there was a “widespread desire” that this expanding universe might someday reverse itself and contract, leading to “some cyclical restoration of activity.” But he was not in sympathy with this view, expressing surprise that it had such a “strong aesthetic hold” on so many people. Here Russell enjoyed a bit of Eddingtonian wordplay to make his point: “I am an evolutionist, not a multiplicationist. It seems so tiresome to me doing the same things over and over again.”<sup>22</sup>

Indeed, this is the same Russell who in 1914 chided a critic’s rejection of his penchant for making inferences based upon statistical behavior and not direct measurement: “A hundred years hence all this work of mine will be utterly superseded: but I am

---

<sup>19</sup>Quoted in DeVorkin (2000, p. 246).

<sup>20</sup>Russell (1949, p. 16). Russell did wonder privately if the heavy elements were older than the stars, in letters to van Maanen and Shapley in 1939. See DeVorkin (2000, p. 254, ref. 83). For a contemporary review of time scales, see Bok (1946). Good secondary sources that address one or more aspects of the time scale problem include Brush (1978b); Dick (2001); Hufbauer (1991); Burchfield (1975).

<sup>21</sup>Russell (1949, p. 29). Russell identified the “outstanding difficulties” remaining as the persistence of giant stars and the premature appearance of white dwarfs. This was at a time before Baade’s populations were articulated, and when giants were still considered, by Russell at least, to be young stars, in spite of the initial evidence provided by Strömgren, Kuiper and Gamow. See DeVorkin (2006).

<sup>22</sup>Russell (1949, p. 30)

getting the fun of it now” (Russell 1914).<sup>23</sup> So even while he could marvel at Slipher’s high radial velocities, van Maanen’s proper motions or Hubble’s vast distances, following the observations to see where they lead, he knew only too well, and was comfortable with the fact, that the implications one might draw from them were fleeting.

Nevertheless, in his last known commentary on the implications of the expanding universe, in a foreword to the English edition of Lemaître’s “The primeval atom, an essay on cosmogony,” Russell kept his distance from the question. Describing it as an exercise in “pure intellect,” as an hypothesis and not a theory, he felt it “goes far beyond the limits of present proof.” Indeed, not apparently aware of, nor concerned with, the recent prediction of a redshifted remnant by Ralph Alpher and Robert Herman in 1948, nor making any effort in his foreword to explicitly link the theory to the observed redshifts themselves and that the universe must be expanding, Russell slyly regarded Lemaître’s retrospective essay as “...a fascinating view of the birth and growth of a noteworthy hypothesis regarding the origin of the material universe.” The only reference Russell made explicitly to observational evidence dealt with the recent detection of high-energy cosmic rays which emulated Lemaître’s “atom.” So as he did throughout much of his career, Russell held highest evidence from physical observation, whether or not it tested his preconceptions. Just why he was asked to provide the foreword, and not someone closer to the subject matter, indicates his prominence in the field of professional astronomy in that day.<sup>24</sup>

**Acknowledgments.** This research has made use of NASA’s Astrophysics Data System Bibliographic Services, and the kindnesses of the U S Naval Observatory Library. The author has also benefited from comments by Robert Smith, Michael Neufeld and Joseph Tenn, as well as by the editors.

## References

- Berendzen, R. 1975, Geocentric to Heliocentric to Galactocentric to Acentric: The Continuing Assault to the Egocentric, *Vistas in Astronomy*, 17, 65
- Berendzen, R., Hart, R., & Seeley, D. 1976, *Man Discovers the Galaxies* (Science History Publications Inc)
- Bok, B. J. 1946, The Time-Scale of the Universe (Council Report on the Progress of Astronomy), *Monthly Notices of the Royal Astronomical Society*, 106, 61
- Brush, S. G. 1978a, A Geologist Among Astronomers: The Rise and Fall of the Chamberlin-Moulton Cosmogony, Part 1, *Journal for the History of Astronomy*, 9, 1
- 1978b, A Geologist Among Astronomers: The Rise and Fall of the Chamberlin-Moulton Cosmogony, Part 2, *Journal for the History of Astronomy*, 9, 77
- Burchfield, J. D. 1975, *Lord Kelvin and the Age of the Earth* (Science History)
- Crelinsten, J. 2006, *Einstein’s Jury - the Race to Test Relativity* (Princeton University Press)
- de La Baume Pluvinel, A. 1917, Recent Progress in Astronomy, *The Observatory*, 40, 327
- de Sitter, W. 1932, The Size of the Universe, *PASP*, 44, 89
- Deland, M. 1928, Margaret Deland (Graywood, Kennebunkport, Maine) to Russell, 14 November 1928. Henry Norris Russell Papers, Princeton University Library. Princeton University Library/Henry Norris Russell Papers
- DeVorkin, D. H. 2000, *Henry Norris Russell: Dean of American Astronomers* (Princeton University Press)

---

<sup>23</sup>Doolittle had rejected Russell’s technique of hypothetical parallaxes.

<sup>24</sup>Russell (1950, pp. v-vii)

- 2006, The Changing Place of Red Giant Stars in the Evolutionary Process, *Journal for the History of Astronomy*, 37, 429
- Dick, S. J. 2001, *Life on Other Worlds: The 20th- Century Extraterrestrial Life Debate* (Cambridge University Press)
- Duncan, J. C. 1929, Letter from J. C. Duncan to V. M. Slipher, 6 July 1929. Slipher Papers. Lowell Observatory Archives
- Fox, P. 1920, Letter from Philip Fox to V. M. Slipher, 10 April 1920. Lowell Observatory Archives
- Hertzsprung, E. 1913, Über die Räumliche Verteilung der Veränderlichen vom  $\delta$  Cephei-Typus, *Astronomische Nachrichten*, 196, 201
- Hufbauer, K. 1991, *Exploring the Sun : Solar Science Since Galileo* (Johns Hopkins University Press)
- Jeans, J. H. 1923, Internal Motions in Spiral Nebulae, *Monthly Notices of the Royal Astronomical Society*, 84, 60
- 1924, Letter from Jeans to Hubble, undated, clipped to Jeans to Russell, 23 October 1924. Princeton University Library/Henry Norris Russell Papers
- 1925, Nebulae, Note on the Distances and Structure of the Spiral, *MNRAS*, 85, 531
- Kaiser, D. 1998, A  $\Psi$  is just a  $\Psi$ ? Pedagogy, Practice, and the Reconstruction of General Relativity, 1924–1975, in *Studies in History and Philosophy of Modern Physics*, 29, 321
- Kevles, D. J. 1968, George Ellery Hale, the First World War, and the Advancement of Science in America, *Isis*, 59, pp. 427. URL <http://www.jstor.org/stable/228492>
- 1971, Into Hostile Political Camps: The Reorganization of International Science in World War I, *Isis*, 62, pp. 47. URL <http://www.jstor.org/stable/228999>
- 1995, *The Physicists: The History of a Scientific Community in Modern America* (Harvard University Press)
- Longfield, B. J. 1991, *The Presbyterian Controversy, Fundamentalists, Modernists and Moderates* (Oxford University Press)
- Moore, J. R. 1986, Geologists and Interpreters of Genesis in the Nineteenth Century, in D. Linberg and R. Numbers, eds., “God & Nature” (University of California Press, 1986), 324
- Paul, E. R. 1993, *The Milky Way Galaxy and Statistical Cosmology, 1890-1924* (Cambridge University Press)
- Russell, H. N. 1903, The Heavens in October, *Scientific American*, 89, 235
- 1914, Letter from Russell to Eric Doolittle, 8 June, 1914. Princeton University Library/Henry Norris Russell Papers
- 1915, The heavens in april, *Scientific American*, 112, 318
- 1916a, Nebulae of dimensions, *Scientific American*, 115, 304
- 1916b, Russell, notes, “Scientific Approach to Christianity” lecture III, undated, circa 1916-1917. Box 72.1. Russell Papers, Princeton. Unpublished lecture
- 1917a, Letter from Russell to Harlow Shapley, 8 November, 1917. Princeton University Library/Henry Norris Russell Papers
- 1917b, Letter from Russell to Pickering, 22 November 1917. Princeton University Library/Henry Norris Russell Papers
- 1920a, Letter from Russell to A. van Maanen, 5 October 1920. Princeton University Library/Henry Norris Russell Papers
- 1920b, Letter from Russell to W. S. Adams, 30 January 1920. Princeton University Library/Henry Norris Russell Papers
- 1920c, Some Problems in Sidereal Astronomy, *Popular Astronomy*, 28, 212
- 1924a, Letter from Russell to E. Hubble, 12 December 1924. Princeton University Library/Henry Norris Russell Papers
- 1924b, Letter from Russell to J. Jeans, 4 November 1924. Princeton University Library/Henry Norris Russell Papers
- 1924c, Toronto Lectures, Transcript for 14th Lecture, 29 February, 1924, Box 103.5. Unpublished Talk. Princeton University Library/Henry Norris Russell Papers



- 1925, Widening the limits of the universe, *Scientific American*, 132, 165
- 1926, On the Evolution of the Stars, n.d. ca. 1926. Box 104.26, unpublished essay. Princeton University Library/Henry Norris Russell Papers
- 1927, *Fate and Freedom* (Yale University Press)
- 1928a, Letter from Russell to Deland, 30 November 1928. Princeton University Library/Henry Norris Russell Papers
- 1928b, Sir Norman Lockyer's Work in the Light of Present Astrophysical Knowledge, in *Life and Work of Sir Norman Lockyer*, 382
- 1929a, The highest known velocity, *Scientific American*, 140, 504
- 1929b, Letter from Russell to C. Lampland, 13 January 1929. Princeton University Library/Henry Norris Russell Papers
- 1935, *The Solar System and its Origin* (The Macmillan Company)
- 1949, Time scale of the universe, *Time and its Mysteries Series III* (New York University Press, 1949), 3 - 30, on 16., 3
- 1950, Foreword, English Edn. of Georges Lemaitre (1950). *The primeval atom, an essay on cosmogony*, v. URL <http://books.google.com/books?id=eSoFAAAAMAAJ>
- Russell, H. N., Dugan, R. S., & Stewart, J. Q. 1938, *Astronomy: A Revision of Young's Manual of astronomy*, v. 2 (Ginn and company)
- Shapley, H. 1918, Globular Clusters and the Structure of the Galactic System, *PASP*, 30, 42
- Slipher, V. M. 1919, On the Spectra of the Orion Nebulosities, *PASP*, 31, 212
- Smith, R. W. 1982, *The Expanding Universe: Astronomy's "Great Debate" 1900 - 1931*. (Cambridge University Press)
- 2009, Beyond the Galaxy: The Development of Extragalactic Astronomy 1885-1965: Part 1; Part 2, *Journal for the History of Astronomy*, 39; 40, 91
- Thompson, L. A. 2011, Vesto Slipher and the First Galaxy Redshifts, *ArXiv e-prints*. 1108.4864
- van de Kamp, P. 1965, The Galactocentric Revolution, A Reminiscent Narrative, *PASP*, 77, 325
- Wright, H. 1966, *Explorer of the Universe: A Biography of George Ellery Hale* (American Institute of Physics). URL <http://books.google.com/books?id=T3wrAAAAIAAJ>